Response to reviewer R1 comments

June 23, 2025

Thank you for your detailed review, we will be commenting on the comments by reviewers R1 and R2 separately and upload full responses to reviewers, tracked changes and a revised manuscript after the discussion phase has ended. In this document, reviewer comments are in **black** and our comments are in **red**.

This is a clearly written paper with nice figures describing nice analysis of an extraordinarily rare and hard to obtain dataset. The manuscript should be published.

Thank you for your positive assessment of our paper, we will address your indiviual points below

I do have a number of comments, questions and morsels for thought that I list below in the order in which I read. The majority are (very) minor, amounting to text and grammar nits, but some are more substantive. In particular I would like to see

- more supporting evidence behind the claim that mixing is weak (for the reasons given in the final comment below),

  Could you elaborate on the type of evidence you are looking for? We show that the median TKE dissipation rate is 10^{-11} to 10^{-10}, which are very low values, comparable to the background TKE dissipation rate in the ocean.

- better figure 3 and 4, which currently mixes aspect ratios, has the reader going back and forth and does not allow direct comparisons of the most relevant quantities - specifically epsilon and the different instability indicators

  We will adjust Figures 3 and 4 to have the same aspect ratio and trial different combinations of the data in our revised submission.

- quantification of the ADCP vertical wavenumber response and hence justification of the numerical values of Ri presented (or alternately toning down the reference to specific values such as $Ri = 1/4$ given the estimates are noisy and not fully resolved),

  We are unsure what the reviewer is asking about here. Do you refer to the vertical wave number response that Polzin et al. (2002) refer to when estimating turbulent mixing processes from vertical shear in the ADCP? We do not use the ADCP to calculate mixing, we only use it to get information on the horizontal velocity in the vicinity of our microstructure shear measurements. The Richardson number is calculated from the vertical shear between successive 8 m tall (in the vertical) ADCP bins, but this is not used for the turbulent shear calculations. Could you clarify what you are concerned about here?

- justification for use of median versus mean

  We use the median as it is less impacted by outliers or non-normal distribution of values. If the data is normally distributed the median and mean are identical, so there is no negative effect of using the median as the default method of averaging values.

- and finally and perhaps most substantively, an explanation for why the turbulent heat fluxes just above the bottom are important to measure. Ie, is that the water that will eventually meet the grounding line, or should the study have been done nearer the top of the mCDW watermass where the gradients and heat losses are much stronger?

We will add information to the text to clarify that we measure heat fluxes close to the bottom to capture the effect of topography roughness on the flow, to capture the mixing where the bottom intensified warm inflow interacts with the seabed and due to practical constraints (the ALR needs to stay within 100 m of the seabed to allow for accurate dead-reckoning and bottom tracking).

Good luck. I enjoyed reading the paper and hope that these comments are useful.

11: topography, turbulent or both not resolved?

We will clarify this sentence to confer that turbulent mixing is not resolved in models and topography is not resolved in bathymetry products or models.

26: awkward

35, 53: "this" is a weak reference. Please reword; see Strunk and White if needed.

48: Melt rates two words?

We will correct this

52: This statement is actually not true: epsilon is the dissipation rate and further assumptions must be invoked to infer the mixing. This needs to be corrected and expanded upon.

We will expand our wording here to emphasis that certain assumptions need to be fulfilled to associate epsilon with turbulence, such as Taylor's frozen turbulence hypothesis.

55: This would be a good place to distinguish what is different about this study from the other two.

We will add words to that effect here.

56: which $->$ that. Also, is this the only reason mixing is important to know for these situations?

mixing at the seabed – ocean interface is also important for nutrient transport, such as the transport of iron from sedimentary sources to the euphotic zone. We refer to such processes in the paragraph above.

66-68: Please give order of magnitude of the clock offsets before correction and the precision of the alignment afterwards.

We will add this information in the revised manuscript.

74: Please explain why you used median instead of mean?

See our explanation above.

95: Could indicate this is likely because of $F = ma$; ie the same force on the huge autos produces much smaller accelerations.

We will add a note in the revised document speculating that the lower impact of vibrations on ALR compared to smaller craft is due to the greater weight of the vehicle.

105: on which this study focuses.

This will be changed in the revised manuscript.

105 general: is this the first paper that presents the details of shear microstructure from Autosub? Surprising if so but if true, you might consider showing a few spectra and additional details, possibly in an appendix, so that future work can cite this paper.

This is not the first such paper, we refer the reader to Davis et al. (2022) for information of the spectral response of the shear probes on ALR. This reference will be added to the revised manuscript.

111: Shih et al is a very bad reference for this! They find a Re_b-dependent Gamma. Suggest just citing Osborn (1980). There are also now a handful of observational references supporting the assertion that $\Gamma = 0.2$.

Thank you for ppointing this out, we will remove the reference to Shih. If you can point us toward the observational studies that find $\Gamma = 0.2$, we would be interested in seeing them.

113: How close to the bottom of the ice is the shallowest CTD measurement shown? The very strong gradients at the very top of the cavity CTD casts (Fig 2 black) are interesting.

The CTD cast goes right to the ice – ocean interface. We refer the reader to Wåhlin et al. (2024) for a discussion of the CTD measurements at the interface.

123 and throughout: I believe units should be in roman, not italicized, font.

We have corrected this where we found such instances, all remaining formatting will be finalized in the copy editing process.

136: Suggest reformatting the equation.

This will be reformatted in the revised document

140: Please make it very clear that Ri (under the ice at least) is based on a single N2 profile whereas the shear is a function of location and time. This is OK, but appropriate caveats as to its governing local instabilities without in-situ N2 should be given.

We will add additional words to this effect to the revised manuscript

173: Generally, avoid "there is" in favor of more active language such as "flow is to the . . . "

We will change some of our wording where we deem appropriate in the revised manuscript

177: High compared to what?

We will add context that epsilon is high compared to the range of epsilon observed at the ice shelf front.

177: runon sentence.

In addition to adding context (see above), this sentence will be split into shorter sentences.

Figure 3, lines 2 and 4 of caption: runon sentences. Also, the dots are said to indicate the starting locations - but they are a continuous line. I'd have thought there would just be two starting locations, one for center and one for east? Please clarify.

We are unsure what you mean by "dots are said to indicate starting locations". There are two large dots with a black outline that show the values at the starting locations of the east and centre tracks. All other dots show the 10-minute median of the values of the "along" dive track. If we make it clear that we plot the 10-minute median of the ALR values along the dive track, would that solve the issue?

Figure 4: Personally I think it would be better to keep the aspect ratio constant between Fig 3 and 4. Also, sine you already plotted velocity in Figure 3, suggest including a panel of N2. The aspect ratio is all the more a problem later when the authors are comparing epsilon to the different instability indicators - but the reader must go back and forth between figure 3 and 4. Suggest standardizing the aspect ratio and including an epsilon panel in Figure 4. Possibly even adding Ri contours to the epsilon panel or epsilon contours to the Ri panel since the authors are trying to demonstrate correspondence between the two quantities.

Thank you for this feedback, we will standardise the aspect ratio between Fig 3 and Fig 4 and trial your suggestions regarding N2, epsilon and Ri in the revised manuscript. However we may decide not to adopt a specific suggestions due to readability or an excessive number of subpanels.

Also, the Ri panel is just a big sea of red. Consider plotting something else to highlight the unstable regions such as Ri^{-1} or $Fr = Uz/N$.

The Ri panel is mainly red due to the choice of colourbar. We chose to plot $Ri < 1/4$, $1 > Ri > 1/4$, and $Ri > 1$ as three different colours in keeping with established practice to distinguish along criteria for instability. Plotting 1/Ri would make it less obvious where $Ri < 1/4$. We would like to avoid plotting additional instability metrics such as the Froude number to avoid confusion.

182: Doesn't negative PV mean unstable? The whole water column is unstable? Is it backwards in the southern hemisphere? Some statements to clarify would be useful.

Instabilities may develop when PV and f have opposite signs, as f is negative in the southern hemisphere, PV > 0 indicates conditions favourable to instability. We will clarify this in the text.

188: I don't agree with this statement - the high dissipation does not appear to me to line up at all with for Ri. Furthermore, given the ADCP's finite vertical resolution and noise, some additional detail needs to be given on how seriously we are to take the numerical value of Ri. I think that either some wavenumber spectra and transfer functions a la Polzin 2002 need to be included, or Ri used as a qualitative indicator.

As far as we understand Polzin et al., 2002 the vertical waenumber response of the ADCP is relevant when calculating turbulent dissipation from the ADCP. We are not using the ADCP for turbulence. We use the VMP or microrider for shear microstructure and the LADCP and ADCP on the ALR to get an idea of the vertical and horizontal structure of the water column at much larger scales, a background value if you will. Ri is frequently calculated from LADCP output with bin sizes of 8 m. If you have additional concerns could you be more specific on how Ri would be affected by the ADCP resolution? And what you mean by "wavenumber spectra and transfer functions"?

191: I disagree; elevated mixing is much broader than the regions of $Ri < 1/4$ - augmenting my previous point.

We will clarify that the high epsilon includes, but extends beyond, the region of low Ri. Can you clarify how this is related to your previous point?

193: This statement is not justified. Epsilon appears surface intensified as well. And while it is bottom intensified, I do not think the statement that it is heightened over rough topography, shear or high currents (of which you generally must choose either high current or high shear, not both...) is supported. And as before, I don't think that high epsilon lines up with low Ri either. Either way, if this statement is retained, more analysis needs to be shown - scatter plots, binned averages, etc.

We will clarify that we are only considering epsilon below the Winter Water layer (approx 400 m), thus we do not discuss high epsilon at the surface. Can you clarify why you think high current speed can not coincide with high current shear? We will include scatter plots in our response to reviewers, however, as we state in the manuscript, the relationship between topography, shear and epsilon that we find around 0 km in the east dive track is not valid everywhere. Additionally, the bathymetry gradient deeper in the cavity is affected by the low resolution of bedmachine, making correlations and scatter plots noisy.

197: runon sentence. And seemingly unrelated sentences. Ri governs shear instability, not symmetric instability... (I understand they are highly correlated here, but they are different, so clarification is needed).

We will insert a paragraph break before "at the nearby Pine Island Ice Shelf....". We will clarify that SI is not governed by Ri.

202: What is a barotropical jump?

It is an oceanographic term for an abrupt change in water column thickness. This occurs at the ice shelf front, since ocean currents want to flow along lines of uniform water column thickness, the ice shelf draft poses a barrier to flow, even at depths deeper than its draft.

207: Please rewrite this passive and vague sentence.

We will rewrite this sentence in the revised manuscript.

204-210: Suggest moving this speculative bit to the discussion.

We originally had results and discussion split, but chose to integrate them to avoid duplicating information and to limit jumping back and forth between topics. We will retain this structure.

216: I think it would be nice to compare this to open ocean values at a similar depth and/or abyssal values, for context. Otherwise "weakly stable" doesn't have meaning.

We will add typical open ocean values for N2.

218: Style guides such as Strunk and White suggest avoiding "Figure x shows..." in favor of "statement x is true (Figure y)."

We will rephrase this sentence.

223: Figure 6 and 5 -> Figures 5 and 6

Thank you for pointing out this typo

236: redundant. Suggest "Maximum values were" or "Values reached."

We will change this in the revised manuscript

238: Again, I'm afraid I don't see this. There are counter examples where epsilon is high over flat bottoms. Please include plots that allow direct comparison such as plotting epsilon with Ri, current speed or bathymetric slope over plotted, or scatter plots or binned averages (e.g. epsilon(Ri) etc) if you want to make this claim.

We will clarify that we are not making statements about a universal relationship. Turbulence is influences by many factors, some only incompletely resolved in our study, or not resolved at all. Our statement is a qualitative observation that can explain many of the observed patches of high turbulence, though you are right that there are regions where this relationship may be absent or obscured by the limitations of our study.

241: Please remind reader that it's Ri computed from in-situ where and

The remainder of this sentence seems to be missing, please clarify

245: Again, please include transfer function and instrument response information if you wish to quantify the numerical value of Ri versus using it as a qualitative indication. Note as well that these transfer functions and hence the mapping of true to measured Ri will be different for the Autosub and the LADCP.

Can you clarify what you mean by the mapping of true to measured Ri and how that is influenced by the ADCP? It is common practice to calculate Ri from vertical shear from the 8 m binned ADCP and we do not use the ADCP to calculate fine scale microstructure.

257: Is it really necessary to use a package like this to compute a spatial gradient? More fundamentally I do not see a relationship between RMS bathymetric slope and dissipation rate.

I do not use a package to calculate the gradient. The bathymetry from ALR is only 1D, to get a 2D gradient I use bedmachine to get the bathymetry normal to and along the ALR dive track. Can you clarify what you mean by "RMS bathymetric slope"? We do not calculate RMS of the slope and to our eyes there is a clear relationship between the bathymetry and epsilon close to 0 km on the east dive track.

264 onwards: consider moving all of this comparison to past work to the discussion, so that the results section just has your results?

A previous draft had results and discussion separated and the feedback from several readers was that this caused unnecessary confusion, duplication and jumping back and forth. We will keep the results and discussion merged.

270: I'm confused here, sorry. Weren't the ALR measurements entirely in the warm inflow, since they were so deep?

We will clarify this sentence, you are correct that all our ALR measurements are in the warm layer of in the cavity, but we define the inflow as the narrow bottom intensified current along the 700 m isobath.

272: runon sentence.

The sentence in line 272 is not that long, are you sure that this is the line number you meant?

273: Due to what mechanism? We will add this information in the revise manuscript.

281: Please change "this" to "their" to avoid confusing with your study.

we will make this change in the revised manuscript.

285: If you are going to state dissipation rates this low, I think you do need to demonstrate your minimum detectability threshold. Earlier you said it was 1e-10. So how then do you get a median lower than this.

The detection limit is between 10^{-11} and 10^{-10} depending on the dive track. We never state that the detection limit is 10^{-10} and will clarify the sentence you refer to.

Again, I think median should be avoided for all quantities unless there is a good reason. Why not just use the mean?

We do not want to cause confusion by switching between mean and median for data with and without outliers or non-normal distributions, since median(x) = mean(x) when x is normally distributed we think median is a better choice.

333: The reason for these calculations is revealed here - suggest giving it earlier to make the reader understand why they are being told all of this. More fundamentally, is that the only reason turbulence is important to measure under ice shelves? Ie, as a possible mitigator of the advective heat flux by these warm flows?

We will move the motivation for the mixing calculation to earlier in the paper. As far as the ice melt rate and modelling efforts in the cavity are concerned the heat flux is the major concern. As we discuss above, mixing is also important for the trace metal and nutrient transport, however we do not have measurements of concentration gradients in the cavity and can not make a statement as to how the mixing influences them.

I, at least as a non ice sheet person, would like to see a cartoon (words or actual graphic) showing a cross section of the hypothesized warm water flow to the grounding line. The reason for this is that I don't currently understand why the study focused so much on the near-bottom mixing. I'd think that the heat loss out of the mCDW would be better quantified near its upper edge. As the authors point out, the water near the bottom is very weakly stratified so the heat fluxes are expected to be small. Aloft nearer the interface, the gradients would be stronger, but also the distance from the topography which is presumably generating most of the turbulence (my comments above about that not having been adequately demonstrated notwithstanding). So, statements that mixing is weak such as on lines 356-258 should be tempered somewhat. And I think the cartoon or written description of the flow giving readers the sense of which depths are thought the most likely to eventually contact the ice would help inform this discussion, at least for me.

We will add a cartoon to the revised manuscript, thank you for the suggestions. As a short description of why the bottom layer of mCDW is so important is that the grounding line (the region where the ice flow contacts the ocean and comes afloat) is the area where most melting and glacier retreat takes place.

References

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